1-1-2000

Richard Goldschmidt and the Crossing-Over Controversy

Michael Dietrich
Dartmouth College

Marsha Richmond
Wayne State University

Perspectives

Anecdotal, Historical and Critical Commentaries on Genetics

Edited by James F. Crow and William F. Dove

Richard Goldschmidt and the Crossing-Over Controversy

Marsha L. Richmond* and Michael R. Dietrich[†,1]

**Interdisciplinary Studies Program, Wayne State University, Detroit, Michigan 48202 and [†]Department of Biological Sciences, Dartmouth College, Hanover, New Hampshire 03755*

ONE of the basic tenets linking the Mendelian laws to the chromosomal theory of heredity was the assumption of "crossing over." On the basis of F. A. Jannsen's description of the "physical twisting or intertwining that occurred between paired homologous chromosomes during the early stages of meiosis" (Jannsen 1909), the notion of crossing over was first employed by Thomas Hunt Morgan in 1911 as a satisfactory mechanical explanation for the factorial recombination found in Mendelian crosses. Crossing over, as Garland Allen noted, thus became a foundational assumption in the subsequent articulation of the Mendelian chromosome theory of heredity. Morgan and his co-workers proposed a "beads-on-a-string" model of the chromosome, with genes represented as particulate beads strung together linearly like pearls on a necklace. Crossing over was invoked to explain the linked traits in terms of the proximity of these particulate genes along a chromosome. Genes that were farther apart on a chromosome had a greater chance of recombination as a result of the exchange between homologous chromosomes. This relationship between frequency of recombination and the arrangement of genes on a chromosome allowed the first chromosome maps to be constructed (Allen 1978). The central place of crossing over in the conceptual arsenal of the Morgan group was evident in *The Mechanism of Mendelian Heredity* (Morgan *et al.* 1915). Indeed, it formed the basis for the Mendelian chromosome theory of heredity and ultimately the theory of the gene.

Allen particularly drew attention to the mechanistic materialism that underlay these conceptions. "At the core of the gene theory of Morgan and his co-workers was a strong mechanistic bias not unlike that which had pervaded physics, chemistry, and physiology in the latter half of the nineteenth century. Classical geneticists tended to see genes as discrete units interacting in predictable ways, just as physicists had seen atoms as discrete units whose interactions followed definite 'laws'" (Allen 1978, p. 51). Not surprisingly, then, the Morgan group responded forcefully when in 1917 Richard Goldschmidt directly challenged their mechanistic interpretation of crossing over.

THE VARIABLE FORCES HYPOTHESIS

In "Crossing Over ohne Chiasmatypie?," published in the second volume of *Genetics*, Goldschmidt did not, of course, deny that crossing over or genetic recombination took place. Rather, he questioned the chiasmatype hypothesis. While praising the work of the Morgan school by noting that they had provided, through the combined analysis of cytological observations and breeding experiments, "the most important enrichment of genetics in the last few years," Goldschmidt nonetheless argued that "this should not blind nor hinder us from applying criticism where it seems necessary" (Goldschmidt 1917, p. 82). It should also be noted that Goldschmidt did not challenge the chromosome theory of heredity nor even the Morgan group's assumption of the linear arrangement of genes along the length of the chromosome, both of which he fully supported. Rather, it was their particular materialistic and mechanistic representation of factorial recombination occurring through the breakage and reattachment of chromosomal segments to which he objected.

The assumption of chromosomal interchange during chiasmatype, Goldschmidt proclaimed, was not only speculative but completely unnecessary. Linkage and factorial exchange could much more easily and satisfactorily be accounted for by assuming the existence of variable forces "anchoring" the genes to the chromosomes. Genetic recombination would thus result from factorial exchange (crossing over). However, the exchange would depend on the strengths of the anchoring forces as well as on the "number of somatic divisions of the germ line." To explain his ideas, Goldschmidt

[1]*Corresponding author:* Department of Biological Sciences, HB 6044, Hanover, NH 03755. E-mail: michael.dietrich@dartmouth.edu

provided an alternative "physicalistic" picture of what he envisaged was happening at the cellular level:

> During the resting stage of the nucleus, the chromosomes physically disintegrate. At the next division, however, they regain their individuality [*finden sie sich aber wieder unter Wahrung der Individualität vor*]. There must, therefore, be some kind of force that comes into play in the formation of the chromosome such that it always instructs each particulate hereditary factor to again find its proper chromosome and its proper place. Whether this is explicitly mentioned or not, it is certain that every chromosome hypothesis is based on this conception (GOLDSCHMIDT 1917, p. 84).

As for the nature of these forces, he was uncertain. "They might be forces of chemical affinity, the effect of forces of mass [*Massenkraft*], or even ordinary mechanical forces [*grobmechanische Dinge*]." The primary consideration was that the force needed to be specific and typical for a given particulate hereditary factor. But not too specific, for it was the slight variability between the forces anchoring different alleles of a hybrid that accounted for genetic recombination. If the two forces were greatly dissimilar in strength, it was less likely that any interchange would take place between the two alleles. Hence, factors on the same chromosome would appear to be "linked." The more similar the two forces were, the more likely that an occasional mistake would arise, with the alternative alleles changing places and thus resulting in genetic recombination.

Like the chiasmatype hypothesis, Goldschmidt noted that the variable forces model could be represented mathematically. However, the difference between the two was that what was being measured was not the distance or "the relations between the morphological position" of factors, but rather the "relative effect" of the respective forces in play. "The amount of crossing over," Goldschmidt noted in summary, "is the expression of the operation of some kind of quantitatively variable force that is responsible for the attachment [*Zugehörigkeit*] of a determinant to one of the chromosome partners, a force whose relative numerical operations naturally can also be represented geometrically as a segment of a line" (GOLDSCHMIDT 1917, p. 84). Referring to the accompanying figure (see Figure 1), Goldschmidt explained: "The force that anchors a particulate hereditary factor to its chromosome is denoted [*wiedergegeben*] by a right-angled anchor, whose size corresponds to the quantity of the force."

Goldschmidt clearly stated that it was not his intention, in proposing such a model, to replace or otherwise argue for discarding Morgan's model. His aim was rather to show that "the same forces that cause the individuality of the chromosomes can also explain crossing-over," and hence that explanations of a "physical-chemical or dynamic nature" were also compatible with known facts and accepted assumptions (GOLDSCHMIDT 1917). Ultimately, then, he sought to substitute a physiological interpretation for a mechanical morphological model. Here, as in his subsequent work, he exhibited an expressed preference for a dynamic rather than a static explanation, which he believed held more promise in explaining the facts of development as well as heredity. Hence, his opposition to the chiasmatype hypothesis and rejection of the mapping of loci.

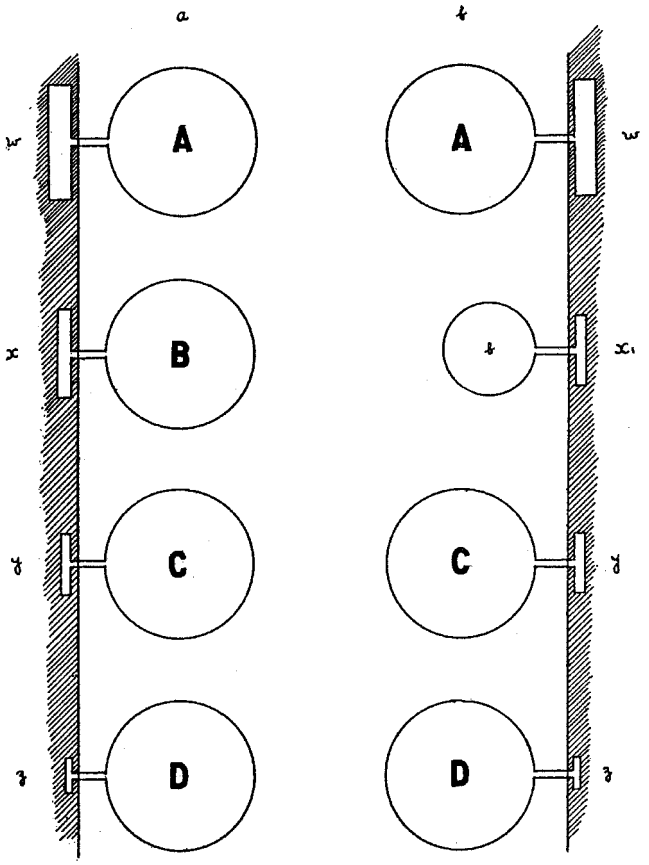

FIGURE 1.—Richard Goldschmidt's representation of the variable forces hypothesis. Particulate factors were depicted as anchored to the chromosome. The size of the anchor corresponded to the force attaching the particle to the chromosome (from GOLDSCHMIDT 1917).

CRITICISM OF THE VARIABLE FORCES HYPOTHESIS

Immediately after the appearance of Goldschmidt's article, Alfred Henry Sturtevant submitted a rejoinder to GENETICS, which was published in the next issue. Entitled "Crossing Over Without Chiasmatype?" (an English translation of Goldschmidt's title), Sturtevant's brief paper criticized the variable forces hypothesis on two fronts—from the standpoint of cytology and on the basis of the mathematics underlying calculations of crossing-over percentages. As for its cytological support, Sturtevant noted that Goldschmidt's model was based on "the idea that the chromosomes lose their structure during the resting stages, so that it is necessary that the particles be reassembled later to form the chromosomes

seen at mitosis." Recent studies, however, left this assumption "open to serious doubt." If this hypothesis were discredited, then so too would be the whole model, "for it is assumed that the same mysterious 'Kraft' is responsible for the rebuilding of the chromsomes and for crossing over" (Sturtevant 1917). Of most concern to Sturtevant, however, was Goldschmidt's challenge to his project of chromosome mapping and the resulting calculations of crossover percentages. To counter Goldschmidt's implication that his data were derived with an incorrect formula, Sturtevant reviewed his procedures, providing data gleaned from "experiments involving three or more loci at the same time." This, he asserted, Goldschmidt had mentioned only summarily, and yet it "really puts the linear arrangement and chiasmatype theories on a sound basis." Goldschmidt's calculations, however, lacked credibility, since they were only "a perfectly obvious application of an elementary principle of probability." Finally, Sturtevant noted that Muller had provided the strongest evidence to date in support of the chiasmatype hypothesis through his recent discovery of "interference," or the demonstration that "one crossover tends to prevent the occurrence of another one near it." In the face of such evidence, he concluded, Goldschmidt needed a more cogent alternative before he could legitimately call the chiasmatype hypothesis into question.

Calvin Bridges, in his response to Goldschmidt's article, took a tack different from that of Sturtevant. He assumed that Goldschmidt's variable forces model held true and then attempted to show that, while it sufficed to explain cases of simple linkage, it had difficulty accounting for crossover data in subsequent generations, or "extended to the results which such crossovers give when bred" (Bridges 1917). Take the case in which the "frequency distribution" between the two forces (F_G and F_g) that Goldschmidt assumed anchored the two alleles G and g to the chromosome was 1%, that is, where crossing over occurred in 1% of the gametes. Then, "let us mate a cross-over individual in which gene G is held incorporated by force F_g with the converse crossover individual in which gene g is held incorporated by force F_G." During the resting stage, according to Goldschmidt's model, the forces are relaxed and the genes freed:

> It must now be recalled that every value of force F_G is a member of a specific frequency distribution representing the entire behavior of F_G, and that any particular value of force F_G should give in succeeding generations the same result as every other value of F_G; . . . the two distributions which describe the variates of F_G and of F_g in the cells of the new heterozygote, being specific, overlap in exactly the same fashion and to the same extent as did the distributions of the forces F_G and F_g in the original heterozygote. . . . Consequently, when the chromosomes are reassembled force F_G will, as before, incorporate gene G in 99 per cent. of cases and gene g in 1 per cent. of cases (Bridges 1917, p. 372).

But, as Bridges pointed out, this resulted in a *reductio ad absurdum*: "*But gene G entered the heterozygote as part of the chromosome possessing force F_G, hence the 99 per cent. of emerging offspring in which gene G is incorporated by the chromosome bearing F_G or gene g by the chromosome bearing F_g are crossovers*" (p. 374). In fact, however, the crossover values from one breeding experiment to the next were always basically equivalent. Thus, he concluded, "Goldschmidt's machine which at the first revolution turned out a mere driblet of crossovers, should overwhelm the operator with a deluge of crossovers at the next turn of the crank. The whole explanation fails unless some added agency be devised to take over the duty which the specific allelomorphic forces abandon after the occurrence of crossing over." The only such hypothesis that might counter this problem was to suggest that the variable forces themselves cross over as well, but this "makes more demand on credulity than, for example, one would in assuming crossing over offhand as a specific property of genes which needs, as support, only such formal explanation" (Bridges 1917, p. 374).

Bridges's assessment of the weaknesses of Goldschmidt's model was fair. The model certainly did not hold up when confronted by data of crossover rates produced in subsequent generations. However, it is perhaps even more interesting to note that in criticizing Goldschmidt's model, Bridges analyzed it by converting it to a mechanistic model. For him, as for Sturtevant and Morgan, genetic recombination was better explained as the product of chromosomal mechanics, through crossing over, than as the product of forces operating in the course of normal cellular processes.

Members of the Morgan school were not the only ones to attack Goldschmidt's variable forces hypothesis. Herbert Spencer Jennings, professor of zoology and a specialist in protozoan genetics, also criticized the theory in 1918. Noting that his paper "arose and took shape during discussions on theories of crossing over in the Seminary on Genetics at the Johns Hopkins University," Jennings's publication indicates just how active the discussion of these ideas was at the time (Jennings 1918). Indeed, in correspondence with Goldschmidt, Jennings acknowledged the great importance of this question. "The cross-over problem is quite open yet, it appears to me; and it seems to lead more directly into the unknown relations of the germinal material than anything else we have before us; hence its great interest."[2]

In contrasting the variable forces hypothesis to the chiasmatype theory, Jennings compared the values of crossover ratios calculated on the basis of Goldschmidt's formula with those obtained from Drosophila experiments. He particularly focused on Goldschmidt's assumption of a correlative distribution in the variable

[2]H. S. Jennings to R. Goldschmidt, 12 May 1919, H. S. Jennings papers, American Philosophical Society, Philadelphia.

forces between a particular pair of alleles. The main problem with this view, in his opinion, was that it did not account well for genes being inherited together as a group. However, this was easily explained by the beads-on-a-string model, since this model assumed that "genes are arranged in a linear series and that the exchange, however it takes place, is between segments of these linear series." Although privately Jennings admitted to Goldschmidt that he had doubts about the "precise mechanism set forth by the chiasmatype theory," he still found it more satisfactory than Goldschmidt's variable forces idea. Yet he was "prepared to abandon this notion" should future findings warrant it.[3] For his part, Goldschmidt found it difficult to accept that the given variations in crossover ratios were what "could be expected under simple conditions of sampling." He rather regarded them as based on "a wrong conception of the facts." As he told Jennings,

> Chiasmatype has never been demonstrated since Janssens. Sturtevant's assertions to the contrary [the hypothesis] is reached by substituting twisting for chiasmatype; but twisting, which seems to be practically universal, does not mean a chiasmatype exchange of segments. In many, if not most cases, twisting occurs in very narrow coils. If this would result in wholesale chiasmatype, the crossing-over theory would not work. If only one or a few breaks occur the mecanic [sic] difficulties of the uncoiling would make it a rather strange phenomenon; moreover the physical necessities for the existence of such a cork-screw condition seem to be rather irreconcilable to an exchange of segments. There is thus far no case known where the twisting is confined to the homozygous sex; most of the known cases even refer to the heterozygous sex. Practically the only stage in which male and female sex-cells behave alike in the details of gametogenesis is the synapsis stage; this stage is therefore especially unfit for crossing-over in only one sex. . . . Thus you will pardon me if I still prefer some type of "variable-force" theory to the crude conception of chiasmatype and the distance of the factors.[4]

Although cytology played a contributing role in the elaboration of both hypotheses, it could not adjudicate their differences. Indeed, Sturtevant's claim notwithstanding, cytologists were not at all certain about what happened to the chromosomes during the resting phase, when they disappeared from view. As late as 1925, in the third edition of *The Cell in Development and Heredity*, E. B. Wilson was able to present growing evidence for the genetic continuity of the chromosomes, but not definitive proof. However, Goldschmidt recognized that the chiasmatype theory and the linear ordering of genes along the chromosome were "a splendid way of visualizing the facts." Hence, in textual revisions to the third edition of his genetics textbook, *Einführung in die Vererbungswissenschaft*, he explicitly acknowledged that alternative explanations of crossing over (like his) "cannot be reconciled with the complicated facts and that to date the chiasmatype assumption, although not yet proven cytologically, best explains the experimental findings" (Goldschmidt 1920). As he admitted to Jennings, however, "the mental reservation remains behind the didactic exigencies."

GENES AS ENZYMES

Part of the reason for Goldschmidt's "mental reservation" concerning the chiasmatype hypothesis can be traced to his understanding of the nature of the hereditary factors. In the final section of his 1917 article, he suggested, inferring from several recent studies, that the heredity factors might be related to "the group of enzymes." While morphologists widely assumed that the hereditary substance "is identical to chromatin," he did not believe that this was likely given "the cytological facts about the transformation of the chromatin in the sex cells." Moreover, he also noted that physical chemists had expressed doubts about this association. The recent literature on ferments seemed to indicate that enzymes were a more likely candidate. It had recently been shown that enzymes could withstand damage better than nucleoproteins and that oxidases, or those enzymes associated with oxidation reactions, are, like hydrolytic ferments, bound up with nucleoproteins. "So, therefore, the thought arises that the task of the chromatin is to adsorb the heredity enzymes, to serve as their skeleton" (Goldschmidt 1917, pp. 93–94).

As early as 1911, Goldschmidt had alluded to the possibility that there may be a connection between the hereditary material and enzymes. When discussing sex determination in the first edition of his textbook on genetics, Goldschmidt suggested that there may be "a certain effective substance" connected with sex determination, "perhaps an enzyme, which exerts a certain influence on metabolism of the developing organism whose sex it causes" (Goldschmidt 1911). Such a supposition seemed natural to a biologist who regarded the phenomena of life from a physiological and epigenetic perspective. Always attempting to correlate new data with broader questions of biology, Goldschmidt eagerly looked to the rapidly expanding frontiers of physiological chemistry and attempted to incorporate the latest findings into biological explanations of cellular functioning.

By his own admission, the chemical physiologist whose views on cell physiology and enzymes in heredity most nearly approximated his own was the American Albert Prescott Mathews, whose *Physiological Chemistry: A Textbook and Manual for Students* became one of the most widely used textbooks of the period (Mathews 1920). In his treatment of the chemistry of the cell

[3] H. S. Jennings to R. Goldschmidt, 12 May 1919, H. S. Jennings papers, American Philosophical Society, Philadelphia. Underlining in the original.

[4] R. Goldschmidt to H. S. Jennings, 20 April 1919, H. S. Jennings papers, American Philosophical Society, Philadelphia.

nucleus, Mathews speculated about the possible mechanism supporting metabolic activities carried out in the cytoplasm. After describing the known chemical phenomena involved in cell functioning, Mathews concluded:

> All these facts indicate in no uncertain manner that substances are present in the nuclear sap which on entering the cytoplasm produce chemical changes there. Not only are respiratory changes stimulated many fold, but also digestion seems to be inaugurated. Autolytic enzymes also evidently become active, either because they are set free from the nucleus, or because the nuclear materials activate, directly or indirectly, the inactive enzymes of the cytoplasm. . . . These phenomena speak for the presence in the nucleus of oxidases and digestive enzymes. Since during cell division these enzymes are set free and at the same time the chromatic elements are in many cases plainly loosing [sic] substance, it is possible that these two facts should be correlated and the conclusion drawn that in the resting condition of the nucleus enzymes of various kinds stick to, or combine with, the nucleic acid and are thus accumulated . . . and that during caryokinesis, and possibly at other times also, they are split off from the acid, become free in the sap, enter the cytoplasm and rejuvenate the cell by digesting its accumulated colloidal material. (Mathews 1920, p. 182)

The resemblance between Mathews's views and the model of cell functioning known as the "chromidial theory" earlier developed by Goldschmidt is indeed striking (Richmond, in progress). Within his evolving conceptualization of cellular functioning, however, Goldschmidt found Mathews's suggestion that nuclear enzymes may "stick to, or combine with, the nucleic acid" of the chromosomes, later to "become free in the sap, enter the cytoplasm" and carry out essential life processes an improvement over his earlier model. Enzyme-like genes adhering to a chromosome skeleton, ready to move into the cytoplasm and there initiate the various reactions associated with morphogenesis, provided a dynamic conception of gene action in ontogeny. Although Sturtevant attempted to disparage Goldschmidt's hypothesis by calling his assumption of variable forces "mystical," in fact, Goldschmidt drew upon recent work of biochemists on ferments, enzymes, hormones, and colloids. In discussing the nature of the attractive forces anchoring the hereditary factors to the chromosome, Goldschmidt had in mind the electrostatic forces of protein adsorption currently discussed by colloid chemists.[5] Hereditary factors that were approximately similar would, in this view, adhere to the same place on the chromosome by virtue of the correlative similarity in their adhesion forces. Similarly, the order would be identical in each chromosome and its homolog throughout different cell stages. "Behind the variability and constants of crossing-over," Goldschmidt speculated, "might not perhaps the variability and constants of the laws of adsorption be hidden [*stecken*]?" (Goldschmidt 1917, p. 94).

[5]Goldschmidt cited the description of adsorption provided in Bechhold (1919), the first German edition of which was published in 1912.

CONCLUSION

Given the cool reception Goldschmidt's views received at Woods Hole Marine Biological Laboratory during the summers of 1915 and 1916, it is not difficult to account for the flurry of papers he published over the course of the next few years. These papers appear to reflect a calculated strategy on his part, not only to make his research better known in the United States, but also to counter a disturbing, new trend he perceived developing in genetics. With the publication of Morgan *et al.*'s *Mechanism of Mendelian Heredity* in 1915, it became increasingly clear that genetics was taking a new direction, diverging more and more from the line of work pursued by Goldschmidt and other neo-Mendelians. Particularly disturbing to Goldschmidt was the threat posed by transmission genetics to a physicochemical approach to heredity. The Mendelian-chromosome theory formulated by the Morgan school, despite their denials and instrumentalist rhetoric, was ultimately based on a notion of discrete, particulate genes (Allen 1978). Their model of heredity and cell organization was preformationist and morphological; as such, it was fundamentally opposed to Goldschmidt's epigenetic methodology and philosophy of genetics. Hence, it was incumbent on Goldschmidt to counter such "pernicious" ideas by presenting an alternative dynamic explanation, both of crossing over and the nature of the gene.

Ultimately, the conflict between Goldschmidt and the Morgan school can be regarded as a kind of struggle for authority between two competing theories, methods, and programs for genetics. The Morgan school actively campaigned for the acceptance of transmission genetics, with all that this entailed, while Goldschmidt attempted to support his own vision of a physiological or developmental genetics. Thus, the debate over crossing over and the theory of the gene was an early turf war in the struggle for ascendency in genetics. It represented a competition for predominance of one or the other of two alternative views of the chromosome—the epigenetic *vs.* the morphological—along with distinctive experimental methods, systems, practices, and cultures (Sapp 1983, 1987; Kohler 1994; Dietrich 2000).

Certainly it is true that Goldschmidt's "holistic concept" of the gene, even at this early date, "offered interpretations that were inconsistent with those of the instrumental reductionist's interpretation," that is, the view of the Morgan school (Falk 1986). But the question remains, in holding such "dynamic" views, did Goldschmidt transgress the limits of the available evidence or his methodological procedure at the time? This does not appear to have been the case. Rather, he drew upon a different philosophical approach to

genetics—that of physiology and epigenetics—that contrasted with the morphological and preformationist approach adopted by the Morgan school. As a consequence, he also looked to a different disciplinary field—biochemistry rather than cytology—for clues about the nature of the hereditary material. Given his earlier convictions, derived from a decade of pursuing inconclusive experimental results using a morphological methodology, his approach seemed both promising and reasonable.

Ironically, it was Goldschmidt's assistant Curt Stern who, 15 years later, after working with the Morgan group at Columbia, published definitive cytological confirmation for crossing over (Stern 1931). As a young assistant in Goldschmidt's laboratory Stern wrote a paper favoring the Morgan group's interpretation and with some trepidation gave a copy to Goldschmidt for his comments. Goldschmidt never commented on Stern's manuscript, but, in 1924, he arranged funding from the Rockefeller Foundation for Stern to visit Morgan's group and investigate the cytological basis of crossing over. While with hindsight, historians might judge that the research program of the Morgan school was ultimately more fruitful than that of Goldschmidt, this was not at all clear at the time. In 1920 there were strong empirical as well as philosophical reasons for Goldschmidt and others to believe that the goals inherited from the nineteenth century—to uncover the basis for heredity and development—were still very much attainable.

LITERATURE CITED

Allen, G. E., 1978 *Thomas Hunt Morgan: The Man and His Science.* Princeton University Press, Princeton, NJ.

Bechhold, H., 1919 *Colloids in Biology and Medicine,* translated by J. G. M. Bullowa. Van Nostrand, New York.

Bridges, C. B., 1917 An intrinsic difficulty for the variable force hypothesis of crossing over. Am. Nat. **51:** 370–373.

Dietrich, M. R., 2000 The problem of the gene. C. R. Acad. Sci. Paris **323:** 1139–1146.

Falk, R., 1986 What is a Gene? Stud. Hist. Philos. Sci. **17:** 133–173.

Goldschmidt, R., 1911 *Einführung in die Vererbungswissenschaft.* Wilhelm Engelmann, Leipzig, Germany.

Goldschmidt, R., 1917 Crossing over ohne Chiasmatypie? Genetics **2:** 82–95.

Goldschmidt, R., 1920 *Einführung in die Vererbungswissenschaft,* Ed. 3. Wilhelm Engelmann, Leipzig, Germany.

Jannsen, F. A., 1909 La théorie de la chiasmatypie. Cellule **25:** 389–411.

Jennings, H. S., 1918 Disproof of a certain type of theories of crossing over between chromosomes. Am. Nat. **52:** 247–261.

Kohler, R. E., 1994 *Lords of the Fly: Drosophila Genetics and the Experimental Life.* University of Chicago Press, Chicago.

Mathews, A. P., 1920 *Physiological Chemistry: A Textbook and Manual for Students.* W. Wood, New York.

Morgan, T. H., A. H. Sturtevant, H. J. Muller and C. B. Bridges, 1915 *The Mechanism of Mendelian Heredity.* Henry Holt, New York.

Richmond, M. L. (in progress) *Richard Goldschmidt and Sex Determination: The Birth of Physiological Genetics, 1900–1935* (in progress).

Sapp, J., 1983 The struggle for authority in the field of heredity, 1900–1932. J. Hist. Biol. **16:** 311–342.

Sapp, J., 1987 *Beyond the Gene: Cytoplasmic Inheritance and the Struggle for Authority in Genetics.* Oxford University Press, New York.

Stern, C., 1931 Zytologisch-genetische Untersuchungen als Beweise für die Morgansche Theorie des Faktorenaustauschs. Biolog. Zentbl. **51:** 547–587.

Sturtevant, A. H., 1917 Crossing over without chiasmatype? Genetics **2:** 301–304.